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Observations on the Yellow Fever. By JOHN STEVENS, *Esq. of Hoboken, New Jersey.*

THE following strictures on yellow fever were written during the summer of 1803, when the city of New York was afflicted with this disease. Having myself nearly fallen a victim to it two years before, my thoughts were occasionally turned towards the investigation of the subject on the recurrence of the disease in 1803. What was then thrown upon paper lay by me till the fall of 1805, when it was put into the hands of the editors of the Medical Repository. From the great press of other more important matter, as well as the great length of the manuscript, it remained in their hands until last spring. Seeing no prospect of its appearance in the Medical Repository, I wrote to the editor of the Medical Museum at Philadelphia, signifying my wish to have it inserted whenever he could find room for it; but, not receiving an answer, I did not then send it forward. However, availing myself of the opportunity, by a friend, of transmitting it, I now make him a tender of it, to dispose of in any way he pleases.

New York, August 25th, 1808.

IT is surely high time that the citizens of New York, Philadelphia, Baltimore, &c. should set about, in earnest, to adopt effectual means of exterminating for ever yellow fever from our shores. No exertions, no sacrifices can be deemed too great. Independently of the loss of so many valuable citizens, independently of the distresses and privations sustained by individuals, the mere pecuniary damage, resulting from loss of time, derangement in business, removals of families, and being subject to quarantine in many foreign ports, is incalculable.

The question respecting foreign and domestic origin still remains undecided. This disease cannot, however, with any degree of candour be said to be indigenous, at least on this side of the Chesapeake. Great pains have, indeed, been taken to prove its appearance and existence in places remote from sea-ports, and where no communication with them could have occurred. We are told that yellow fever has raged as violently in the vicinity of the Genesee lake, as it has done at any time in New York or Philadelphia. That fevers of a very malignant type occasionally prevail, and often prove very fatal, in various parts of our country is an unquestionable fact. But of the identity of any of these with yellow fever we have no satisfactory evidence. Nay, I never have yet heard of a single unequivocal case of yellow fever which could not be traced to some sea-port town where the disease prevailed.

If this disease is indigenous, why is it confined exclusively to sea-port towns? And, in these sea-port towns, why does it originate no where but at the wharves? Bilious fever, remitting and intermitting fevers, dysentery, &c. prevail occasionally in all parts of our country. And, I believe, it rarely hap-

pens, during the sickly season, that these diseases do not make their appearance somewhere. Whereas it is a notorious fact, that for fifty years immediately preceding the year 1793, the yellow fever had not appeared any where in our country, not even in the cities of Philadelphia or New York.

In the year 1665 the plague swept off 67,000 persons in the city of London, yet no one ever dreamt that the plague was indigenous in the island of Great Britain. Were yellow fever really generated amongst us, were it merely the effect of noxious exhalations arising from animal and vegetable substances in a state of putrefaction, would it not occasionally occur in different places, whenever these processes, from accidental or other causes, were brought into action? But it is pretended by some that yellow fever, and even the plague itself, are merely higher grades of our common autumnal fevers. Never, perhaps, since the first settlement of the country by Europeans, have fevers and agues, intermitting, remitting, nervous, and bilious fevers, been so universally prevalent throughout the whole of the spring, summer, and fall months, as during the present season. Yet I now challenge the advocates of domestic origin to produce a single unequivocal case of yellow fever originating out of the cities of New York and Philadelphia. Surely throughout so wide an extent of country, during a season so remarkable for fevers of every kind, it is strange indeed that this higher grade should have manifested itself no where but along the wharves of New York and Philadelphia. I am, however, very far from intending to assert dogmatically that our country and climate are incapable of generating yellow fever. All I insist upon is, that from a fair and candid view of facts, this sort of fever ought not to be pronounced indigenous or endemic. But, on the contrary, as it always originates on ship-board, or in dwelling-houses and stores in the immedi-

ate vicinity of wharves, the probability is that it has, in every instance of its occurrence, been derived from a foreign source.

But we are told that the disease is not contagious, and, therefore, cannot be imported. Reiterated experiments seem to warrant the conclusion, that yellow fever is in no case communicable *immediately* from one person to another, in like manner as small-pox, measles, &c. Great numbers of subjects, dying of this disease, have been dissected with impunity. Nay, even the matter of black vomit has been tasted without any bad consequences.

To bring forward individual cases to prove that this disease is not communicable *immediately* from one person to another is at this time wholly unnecessary. They happen every where. The fact, indeed, seems now to be so well established, that the sickening in the country of fugitives from our cities with this disease no longer excites alarm. Even in the hospitals of New York and Philadelphia very few instances have occurred of physicians or nurses taking the disease. And these few instances (in situations where a minute attention to cleanliness is not always to be expected) may be fairly attributed to effluvia arising from articles of clothing, bedding, &c.

But if the non-contagious nature of the disease is thus explicitly admitted, every idea of importation must also be abandoned. This, it must be confessed, is the strong hold of the defenders of domestic origin. Behind this rampart they remain invulnerable to every assault that can be made against them by contagionists. But if it can be made appear that contagion, in the common acceptation of the word, is not necessarily connected with importation, this defence of domestic

origin becomes no longer tenable. To do this I shall presently make an attempt.

It will not, surely, be pretended that the local situations of our sea-port towns are more peculiarly favourable to the generation of yellow fever than other places. Why, then, are these particular spots only, in so wide a range of country, infested with this disease? I shall be told, in answer to this question, that it is owing to the greater accumulations of filth in our cities than is to be found elsewhere. But the advocates of domestic origin do themselves admit that this answer is by no means satisfactory. Mr. Webster declares, "that it is not true that filth and vegetable putrefactions will, in ordinary years, produce this plague." Indeed this concession, to preserve any degree of consistency, was unavoidable on his part. For why had not similar causes produced similar effects, during the long period of fifty years immediately preceding the year 1793? Will it be pretended that since 1793 a total relaxation has universally prevailed in our sea-port towns, and that an unusual accumulation of filth has been suffered to take place everywhere? Mr. Webster, therefore, is perfectly correct when he tells us that filth and vegetable putrefactions will not produce yellow fever. Hence, to account for the appearance of this extraordinary phenomenon, he resorts to a general theory, which he has laboured hard to establish, viz. that pestilential diseases are everywhere the effect of some extraordinary periodical vicissitudes. Now, however he may have succeeded with respect to influenza, measles, scarlet fever, &c. he certainly fails when he attempts to account for the recurrence of yellow fever in our sea-port towns upon the same principles. Let us take his own words. "The autumnal bilious fever is governed by analagous principles, usually appearing in the form of an intermitting or remitting fever,

which is curable, and not infectious, or very slightly so—but in particular years assuming the malignancy of the plague, like the dysentery and ulcerous sore-throat." But where does this ever happen? Not, surely, in the country we inhabit. Where do we ever see "intermitting and remitting fevers assuming the malignancy of the plague?" During the spring, summer, and present autumn, intermitting and remitting fevers have prevailed very generally through the country, but I have never heard of a single malignant case, which has borne the least resemblance to yellow fever. If, from some "extraordinary vicissitude" of season, intermitting and remitting fevers are made to assume so malignant a type in the cities of New York and Philadelphia, surely some symptoms of this malignancy would manifest themselves in other places also. Mr. Webster would not, in this instance, confine the effects of vicissitudes of seasons to mere points, whilst on other occasions, particularly in the instance of influenza, scarcely a quarter of the globe escapes. Effects are ever proportionate to their causes. A general cause must produce a general effect. But that the appearance of yellow fever amongst us does in no shape depend on the vicissitudes of seasons, is evident from the circumstance of its having prevailed for these ten or twelve years past in some one or other of our sea-port towns every season. During this period too we have had wet summers and dry summers, hot summers and cool summers, but without any manifest effects. In short, it may fairly be concluded that this disease is an exotic and not indigenous, or it would certainly show itself in other places besides sea-port towns, and that too at shorter intervals than half a century.

And notwithstanding all Mr. Webster has advanced to induce us to expect a recurrence of yellow fever in our cities at short intervals of time, yet, if it can be kept out for fifty years,

I see no reason why it may not for five hundred. At what interval of time, I would ask, are we to calculate upon a recurrence of the plague in England? It is now nearly 140 years since the great plague, as it was called, appeared in London. Are the periods of its recurrence analagous to the revolutions of some of the comets? No man in 1793 would have ventured to advance this doctrine of periodical recurrence. But, if after a lapse of fifty years, it was then highly reasonable to conclude it a disease of foreign growth, nothing has since occurred to invalidate this opinion. If it was then highly probable that the disease was imported, its recurrence in every instance since, ought certainly to be imputed to a like cause. Since that time our intercourse with the West India islands has been much more intimate and extensive than it had ever been before; and since that time, too, this disease has prevailed in these islands more generally and with more virulence than was ever before experienced. Since the war we have become the carriers of the produce of the French, Spanish, and Dutch colonies. If it is, as indeed Mr. Webster himself admits it to be, an importable disease, is it, I say, to be wondered at that it should from time to time have been introduced amongst us? Indeed, taking all circumstances into consideration, it would have been next to a miracle if it had not.

Let me now bestow a few words on the nature of this disease. Mr. Webster adopts the opinion of Dr. Rush and other physicians, who conceive yellow fever to be only a higher grade of our usual autumnal intermitting and remitting fevers. He tells us that the autumnal bilious fever usually appears in the form of an intermitting or remitting fever, but in particular years assuming the malignancy of the plague. Hence he denominates yellow fever "the bilious plague." I am no physician, but I will take the liberty to assign a number of reasons

which convince me that yellow fever bears no affinity to intermitting or remitting fevers, or to our usual autumnal bilious fever. These fevers prevail occasionally throughout every part of our country; whereas yellow fever is a disease which never appears any where but in sea-port towns. Intermittents are very common in the spring, disappear about midsummer, and re-appear in the fall: whereas yellow fever never appears before midsummer, at the very season when intermittents usually cease. Nay, there seems to be a repugnancy between the two diseases; for an intermittent is now generally considered to be a protection against yellow fever. Persons of diametrically opposite temperaments are the usual subjects of the two diseases. Yellow fever attacks the middle-aged man of a plethoric sanguine habit; whereas women and children, and those who from poverty of living, or from other causes, have poor blood, are the usual subjects of intermittents. Surely, if these maladies were merely different grades of one and the same disease, a predisposition to the one would prove so also to the other.

It is not true that in yellow fever the bile is the primary seat of the disease, which appears to be universally the case in intermitting and remitting fevers, in short, in all disorders arising from marsh effluvia and vegetable putrefactions.

All malignant fevers are more or less attended with eruptions on the skin, which indeed serve as discriminating characteristics of the disease. Yellow fever has also its peculiar species of petechiæ.

I shall not presume to go into any investigation respecting the constituent principles of that specific gas which causes yellow fever. The subject seems yet to be enveloped in obscu-

rity, concerning which the learned have advanced the most contradictory opinions. Thus far, however, I shall venture to state an opinion, that, whatever it may be, it is not the product merely of animal and vegetable putrefactions, because, as I have more than once urged before, the disease would not, in that case, be confined merely to sea-port towns, but would manifest itself generally through the country, wherever processes of this nature occurred.

The cause or causes of yellow fever, whatever they may be, are exclusively confined in their operations to the places where it prevails. During the present season there has not been the most remote analogy subsisting between this disease prevailing in the city of New York, and the diseases prevailing in the country round it. Does not this remarkable circumstance lead us irresistibly to this natural conclusion, that yellow fever as well as plague, beyond the tropicks, are everywhere to be considered as exotics; but, like hot-house plants, though they may be made, under favourable circumstances, to strike root and even to flourish in colder regions, yet, when exposed to the native rigour of the climate, sicken and die?

The introduction and prevalence of the plague were infinitely more frequent formerly throughout the southern parts of Europe than at this day. This exemption at present is unquestionably owing principally to the superior vigilance and rigour with which quarantine laws are now enforced in these countries; perhaps it may be owing in part to improvements in police regulations, and in the mode of living. Colonel Wilson informs us, that "the plague has long been supposed to have been brought from Turkey in the ships charged with old clothes, which constantly came to Alexandria for a market. But the plague has generated annually in Egypt during the

last four years (although no such communication has been possible), and even chiefly commenced in Upper Egypt." "In Cairo, last year, forty thousand people were supposed to be infected with the plague, and many of the French garrison died in that city, although the disease was treated in their hospitals with the greatest ability. In Upper Egypt, sixty thousand of the inhabitants perished during the same season. There whole villages were swept away, and remained abandoned when the Indian army descended the Nile."

Here then are we furnished with the most incontestible evidence of the plague being an indigenous disease in Egypt. It raged everywhere in the *villages* throughout the *interior* parts of the country, so much so that in Upper Egypt sixty thousand perished in one season. Contrast this universality of plague in Egypt with the contracted locality of yellow fever in the United States. Were all the ground now occupied by the towns in the United States, where yellow fever has ever appeared, annihilated, would any perceptible chasm be discovered? Not a hundred thousandth part of the territory of the United States would be lost, yet in such an event would yellow fever be totally extirpated. Can this be true, and the disease with any degree of propriety be called indigenous? Colonel Wilson further says, that during this season the plague did not show itself either at Rosetta or at Alexandria. That these towns should escape in such a season, would indicate a probability that plague in these places generally derived its origin from importation.

(To be continued.)

An Essay on the Partial Inversion of the Uterus. *By* William P. Dewees, *M. D.*

PREGNANT and parturient women are peculiarly liable to sudden and dangerous diseases; it would therefore seem a duty incumbent on every practitioner to relate whatever might tend to lessen or prevent their recurrence. The uterus is subject to such variety of derangements, that almost every day affords something new to the accoucheur. Of this kind is the partial inversion of this viscus. From the nature of the functions of this important organ before pregnancy; from the duties imposed on it by conception; from the efforts which it must exert at the ultimate period of gestation, we are obliged to regard it as one of the most interesting viscera of the female system. But unfortunately this very importance subjects it to peculiar diseases, many of which are so sudden and dangerous, that, if not almost instantly remedied, the patient inevitably dies.

As far as I am acquainted, no one has noticed the partial inversion of the uterus: many cases are on record of its complete inversion and protrusion from the vulva, all of which, as far as my recollection serves me, have proved fatal; but no mention is made of death from its being partially inverted, and where this viscus is still confined to the cavity of the pelvis. Four cases of this kind have fallen under my notice within the last eighteen months, for the detail of which I shall make no apology, as their importance, I trust, will completely justify their promulgation, although it may not warrant my speculations on the subject; but these shall have the merit of being short, if they be not interesting.

By partial inversion I mean where the fundus of the uterus has passed either through the os externum, or is turned down inside out as far as the neck of this viscus. This takes place, I am disposed to believe, but at the full or very near the full period of gestation, as before this time the uterus is not sufficiently distended to subject it to this accident.

For the inversion to take place several circumstances must combine: first, the body and neck of the uterus must remain flaccid, and the fundus contract after the expulsion of the child*; and secondly, it may be essential that the placenta be ingrafted on the fundus.

The remote cause of this disease is, whatever may prevent the contraction of the uterus. The uterus may lose its contractile power from over-distention; this may happen from an excess of the liquor amnii, from the unusual size of the fœtus, or from compound pregnancy; it may lose this power from hæmorrhagy, either before or immediately after delivery; from passions of the mind; from exhaustion by previous disease; from external violence; from its long-continued efforts to effect delivery, &c.

The proximate cause is, whatever may be capable of drawing the fundus down while the remote cause exists: this may be the placenta ingrafted at this part, or perhaps, in some cases, the mere weight of the fundus itself.

* My reasons for this opinion are: first, when the disease exists, we find the tumour very firm, while the body and mouth may remain flaccid some time; and, secondly, because we find the placenta detached, which probably would not happen had not the fundus contracted so as to throw it off.

The indications are simple: the reduction of the fundus, when it has not passed too far through the mouth of the uterus; and, when passed too far for restoration, to take off the stricture occasioned by the mouth through which it has passed contracting too forcibly on the body*, and thus producing disturbances and consequences similar to those which arise from a portion of gut being strangulated.

The first indication is to be fulfilled by placing the back of the fingers against the tumour after the placenta is removed, and pushing it in the direction of the axis of the uterus until the fundus is restored to its natural situation†. Should we find the body of the uterus too flaccid to retain the fundus in its proper place, we ought gently to stimulate it by rubbing the fingers against it until it contracts sufficiently; nor ought the hand to be withdrawn until this effect is produced. I do not know from experience that this extreme torpor may exist in the case we are speaking of, but think it possible; we shall have therefore two difficulties to contend with, in this situation of the uterus, namely, a disposition in the fundus to prolapse, and, secondly, hæmorrhagy, both of which will perhaps be better obviated by the irritation produced by the presence of the hand than by any other means. But, in case of alarming hæmorrhagy, I would not solely depend on the presence of the hand to produce contraction, but would give the acetate of lead freely, and have a stream of cold water poured on the abdomen. We have nothing to apprehend from this procedure,

* See cases III and IV.

† By fundus is to be understood all that portion of the uterus above the insertion of the Fallopian tubes; by body, all that portion between them and the neck.

as it is warranted by experience in hæmorrhagy, not attended with a prolapsus of the fundus.

As soon as the fundus is completely pushed up, and we perceive the uterus to contract, we may safely withdraw the hand.

The second indication is to be fulfilled by grasping the tumour firmly, and drawing it towards the os externum pretty forcibly; by this means we make the body of the uterus pass through its mouth, which is the contracting part. This, I believe, will always be easily effected, as the prolapsed part passes from a greater to a lesser bulk, in proportion as we approach the mouth; for the uterus, as soon as emptied, will return more or less to its pear-like shape. It may be proper to observe, if this case be of any standing, and the bladder not empty, the urine should be drawn off by the catheter. See case II.

This disease may be suspected when the following circumstances obtain: first, where we find the placenta very bulky and firm at the os externum soon after the expulsion of the child, and that it gives more than ordinary resistance to delivery when it is attempted by an exertion at the cord: secondly, when we have applied as much force as the cord ought to bear when the placenta is thus low in the vagina, and we do not find it advance; if the patient complain of much pain from this exertion; and, more especially, when we attempt to aid the force applied at the cord, by hooking the placenta with a finger, and still find it give uncommon resistance: thirdly, when the patient complains of much pain, has some hæmorrhage, is very faint, has cold sweats, and becomes extremely pale, more especially when this paleness cannot be accounted

for from the quantity of discharge*. Under these circumstances, we ought to desist from all attempts to deliver the placenta, until we examine whether the fundus be not prolapsed with this mass. For this purpose, we should either pierce the placenta with the fore finger of the left hand, and tighten the cord with the right, or should search for an edge of the placenta, and trace this to the place of adhesion; if we find there a round, solid, and rather rough surface, we may be sure that the difficulty to the delivery of the placenta arises from a prolapsus of the inverted fundus. Having ascertained the nature of the difficulties to be overcome, we introduce the left hand, if the patient be on her right side, and the right should she be on her left side, or either if on her back, and carefully separate the placenta by insinuating the fingers between it and the uterus: after it is separated it may be withdrawn by the hand that is without; we then proceed, as has been already directed, to restore the fundus.

We are generally cautioned against any violent attempts to deliver the placenta: this advice I conceive founded on just principles, but not precisely on the one for which it seems especially recommended. The only object of this admonition, with the generality of those who have urged it, is to prevent an inversion of the uterus; but this fear is founded much more in theory than in practice, in the generality of cases, as it would be necessary to this accident, that the placenta should be ingrafted on the fundus, and the uterus to be in a state of atony. But experience constantly proves that the placenta is not more (nay, I may very certainly say not so frequently) attached to the fundus as to other portions of the uterus; and if it be not attached to this part, any force applied to the funis could not

* See case IV.

produce the inversion, since it could not act in the direction of the axis of the uterus, through which the fundus must fall. Indeed, I believe it may take place even after the expulsion of this mass, from the simple weight of the fundus itself. But I do not believe this accident can happen when the placenta is attached to the body of the uterus, at least while it preserves that attachment, as this mass will serve to keep up the fundus. Now, as I never would have the rule dispensed with, as the circumstances favourable to an inversion may combine, and would only require a small force in addition to the weight of the placenta to produce it, I would urge that we should not exert any force on the cord until we have ascertained, by externally examining the abdomen, that the uterus had contracted. I by no means believe a force on the cord to be absolutely necessary to produce this terrible disease, yet I can very readily conceive, that in a given case (where the atony was either not very great, or only partial) this force, other things being equal, might bring on an inversion, from which, had it not been applied, the woman might have escaped. The cases I am about to relate are, I conceive, proofs of the first of these positions; for it appeared, on strict inquiry, that no force had been applied by the midwives in cases I, II, and IV, and, as case III was entirely under my own management, I am enabled confidently to assert, that force is not necessary to this species of prolapsus. I am the more anxious to insist on this point, that practitioners, as well male as female, may not be indiscriminately blamed when this untoward case may happen. I would farther urge, that where more than ordinary force appeared necessary to deliver the placenta, or, in other words, if the placenta, when plainly felt in the vagina, cannot be delivered by the force commonly proper to be employed, that we should not persist in this force, until we had satisfactorily ascertained that the fundus was not prolapsed with the placenta.

From four cases of this disease having happened in such quick succession, I am disposed to believe this accident to have happened more frequently than we might at first sight be willing to allow, as no mention (as far as I know) is made of it by the writers on this subject. May we not reasonably trace many of the fatal hæmorrhagies after delivery to this source? may we not attribute other instances of death to this cause, where the disease was neither sought for nor suspected? do not some of the fatal syncopes which sometimes take place after delivery depend on this state of the uterus?

CASE I.

On the 2d of July, 1807, at ten o'clock, A. M. I was called to the wife of Samuel N——, in labour with her first child. Her pains were weak and irregular, but pretty frequent; presentation perfectly natural. As every thing appeared promising, I left her to the care of her midwife. At four o'clock, P. M. she was suddenly delivered; considerable hæmorrhage with faintings followed. I was again sent for, but did not see her until six o'clock, as she lived at some distance from the city. I found her without pulse, cold, and covered with perspiration; with laborious and hurried breathing; the placenta not delivered, and the hæmorrhage continuing. I ordered her such remedies as appeared most pressingly indicated, and immediately examined her per vaginam. I found the placenta just within reach of the finger, and attempted to withdraw it, but it gave great resistance and extreme pain. I now introduced my hand, and found a tumour resembling in shape and size the indentation at the bottom of the common black bottle, over which the placenta was spread. This case was perfectly new to me, although I strongly suspected the nature of the disease. I searched for the detached portion of the placenta,

from whence the flooding proceeded, and carefully detached this mass from the tumour; I then endeavoured to push up this body, but quickly desisted, from the extreme pain it occasioned, and the uncertainty that it was the proper mode of proceeding to procure relief. My patient died in half an hour.

I obtained leave to inspect the body, and Dr. Rush very kindly accompanied me. It proved, as I had previously suspected, to be a partial inversion of the uterus. I dissected out the uterus, which was still so flaccid as to be turned inside out with as much facility as a soaked bladder. The fundus dipped into the body of the uterus about three inches.

REMARKS.

The extreme situation in which I found this patient, renders it very doubtful whether the reduction of the uterus would have been attended with any advantage, but had I had the knowledge of the disease that I now have, I certainly should have attempted it. It may appear to some who speculate on these subjects in their closets, that I failed in enterprize; but let it be recollected the disease was perfectly new to me; that the poor woman was absolutely in articulo mortis; that the pain of the attempt was extreme; and at the moment I believed, that even reduction, were it a prolapsed fundus, would be unavailing, will prove an apology for not persisting, and prevent the charge of suffering a patient to expire before my eyes, when there was a chance of relief. The death of this poor creature was more owing to the immense loss of blood, than to the prolapsus; and the hæmorrhag must be considered as proceeding from the uncontracted state of the uterus. It may be asked how this could happen: the uterus be in a state of relaxation, sufficient to give

rise to a fatal hæmorrhagy, yet offer so much resistance to the reduction of its fundus? The answer is at hand. It is well known that the different parts of the uterus may be at one and the same time in opposite conditions; that is, one portion may be in a state of contraction, while another may be in a state of relaxation. See Baudelocque, p. 146, vol. I. Thus, then, I conceive this case to have been; the fundus of the uterus is never, I believe, sufficiently ample to receive the whole of the placenta, consequently portions of this mass will be attached to its body; this part, from some cause not sufficiently obvious to mention, was at the moment of delivery in a state of atony (or, as Baudelocque emphatically calls it, syncope); the weight of the placenta dragged the fundus through the flaccid walls of the body, while the fundus retained its power of contraction; this contraction would separate a portion of the edge of the placenta from the body, and thus expose the vessels that were before shut by its attachment; some of these vessels are large, and will in the course of a short time pour out an immense and deadly quantity of blood. The uterus will recover its contractile power sometimes even in the moment of death. This I believe to have happened in this case, as the fundus itself was very firm, and the body, as I have already observed, gave a resistance not to be overcome by the force I used. I did not employ much power, but more than sufficient, I am certain now (from experience), to have carried the fundus through the body had it been still flaccid. In case III, I succeeded in the reduction of the fundus with a force not greater than that employed in case I.

The uterus may regain or retain its power of contracting in the moment of death*; it may also lose it again at this mo-

* See Harvey, Baudelocque, &c.

ment. This took place in this case, for at the time of opening the body it was in every part perfectly flaccid. Does this not prove that its action does not depend on elasticity, as has been asserted by some? does it not prove its muscularity?

CASE II.

On Friday, 24th March, 1808, at half past 5 o'clock in the morning, Mrs. P. was delivered of a living child; her waters discharged themselves six or seven hours previously, and before her midwife was called. The placenta came away spontaneously, as the midwife asserted, and to which the patient herself agreed; its expulsion was attended with great pain, and great flooding; she vomited severely for an hour, and several times fainted without an abatement of the discharge. This, however, was eventually moderated by the acetate of lead, and perhaps contraction of the uterus itself.

After this she continued pretty tranquil, but weak, until Sunday morning, when there was a renewal of the hæmorrhagy, with pains resembling those of labour. These ceased in the afternoon; but she became more alarmingly ill. She now fainted frequently, and the discharge continued. In this way she kept until Tuesday, at which time I was called, at the desire of Dr. Atlee, whose patient she now was. The doctor suspected the true state of this woman's case, and mentioned his opinion to me, to which at first I could not assent, as all the cases I had ever heard or read of, as well as I recollected, had proved fatal almost instantly; and the case I had witnessed but a few months before but served to make me doubt the doctor's representation, or rather opinion. Here, were his judgment correct, was an instance of inverted uterus of four days' standing: a case giving contradiction to all I had heard

or believed on the subject. I however visited the patient by appointment, and found her almost exhausted; her pulse so frequent as not to be numbered, and so small as scarcely to be perceived; great difficulty of breathing, and became faint on the least motion; insatiable thirst, frequent vomiting, cold extremities, and a continuance of uterine discharge. I examined her, and found, as Dr. Atlee had declared, the uterus to be inverted. The fundus was down at the os externum, and could readily be seen partially covered with a thin coagulum of blood when the labia were separated. The places not hid by this coagulum were rough or spongy, and of a dark brown colour.

A very dreary prospect presented itself by ascertaining this poor woman's situation; we believed death to be inevitable. But one resource offered itself, namely, to attempt the reduction of the fundus, hoping, as the uterus had not escaped from the vagina, the inversion might not be so complete as to render this impossible. We accordingly proposed this attempt to the husband and friends of our patient, candidly stating her situation, and the almost certain result if relief was not obtained in this way. They without hesitation submitted the case to our management.

We carefully drew her to the side of the bed, and had the knees drawn up and supported. I gently introduced my hand under the tumour, and gradually raised it; this gave me sufficient room to examine the nature and extent of the inversion. The instant I raised the womb there was a large and sudden discharge of urine: this gave still more freedom to an examination that was to terminate in the disappointment of my hope of the reduction of the fundus. I found so much of it had passed through the mouth of the uterus as to render any attempt at reduction futile, and the more especially as the tumour

was augmented by its having swelled since its prolapsus. The stricture occasioned by the contracted mouth was readily felt, and was very strict. I was extremely perplexed for the moment how to proceed, or to announce the failure of an attempt that alone at first sight appeared to promise success or even relief, but it fortunately occurred to me, before I withdrew my hand, that I might take off the stricture by inverting the uterus completely. Agreeably to this suggestion, I grasped the tumour firmly, and drew it pretty forcibly towards me, and thus happily succeeded in slipping the remaining portion through the constricting mouth. The woman was almost instantly relieved from much of the anxiety and faintiness she had before experienced; but as she was so exhausted by previous suffering and discharges, and as the internal surface of the uterus was now exposed to the influence of the external air, I was prevented from feeling or giving the slightest encouragement of recovery to her friends; but fortunately the event proved how groundless were my fears, for from this day she rapidly recovered, without another alarming or troublesome symptom.

Milk was freely secreted on the fourth day after, and continued freely. Our patient was twenty-three years of age, delicate, but always healthy, but more especially so during her pregnancy.

I visited this patient to-day, November 26, 1808, and found her at the wash-tub, perfectly well; suffers no inconvenience whatever from the uterus; menstruated regularly for three periods; had more or less discharge of mucus tinged with blood for four months; this last four months has had no discharge of any kind; suckles her child, which is remarkably thriving. The uterus is so much contracted as to be no longer within reach of her finger.

REMARKS.

In this case we see with what wonderful facility parts accommodate themselves to new situations; the mouth of the uterus is now within the abdomen, while the once internal surface of this viscus is subjected to the action of the external air, but whose influence it appeared to resist for some time, as it persisted for three months in the regular secretion of the menstrual blood. Nay, we do not know whether this is stopped even now by any change effected on its now external surface; it may be the natural interruption from suckling. May this woman again conceive? I do not believe it impossible. It is a case well worth watching, for should this woman again prove pregnant, it will effectually settle a long disputed point of physiology; it will incontrovertibly prove that the semen is not conveyed through the os tincæ to the cavity of the uterus, from thence to the Fallopian tubes, and from thence to the ovaria, to produce conception.

CASE III.

On the 23d of November, 1808, Mrs. G—— was suddenly delivered of a large female child, which breathed and cried freely immediately after its birth. The funis was not cut until after the pulsation in the cord had entirely ceased, which was in about ten minutes. After the child was taken away, I took hold of the cord, and merely tightened it, on which she begged me to wait, as it gave great pain. I, however, traced the cord to the vagina, and found at the os externum a placenta I thought unusually dense and large. On gently attempting to withdraw it, as I thought it loose in the vagina, I found uncommon resistance, which I attributed to its bulk, and desisted

from farther effort, hoping the uterus would by contracting push it completely down. In this I was disappointed ;—some hæmorrhage ensued. I now expected a more than common cause detained the placenta in the vagina, and began a more minute examination. I pierced the substance of the placenta with the fore finger of my left hand, and tightened the cord with my right ; beneath the placenta I perceived a round hard substance, which I but too quickly discovered to be the fundus of the uterus inverted. I immediately introduced my hand into the vagina, and found the detached edge of the placenta from which the discharge proceeded. I carefully separated the whole of this mass, and withdrew it from the pelvis without the least difficulty. A considerable flooding ensued.

As Mrs. N——'s case (case I) gave me a complete insight of the mechanism of this displacement of the fundus of the uterus, and as I had resolved to attempt its reduction if ever an opportunity again offered, I instantly, after withdrawing the placenta, introduced my hand, and pressed the prolapsed fundus firmly with the back of my fingers, and carried it upwards in the direction of the axis of the uterus, and in less than half a minute succeeded completely in restoring it. Mrs. G—— has not had a single unpleasant symptom.

REMARKS.

The success attending this case warrants, I conceive, the hope that this formidable disease may always be relieved, if means be promptly used. It points out the necessity of a careful search in the vagina, where unusual difficulty attends the expulsion of the placenta; where there is hæmorrhagy, and the placenta found at or near the os externum ; but, above all,

when great pain is felt when any force is exerted on the umbilical cord.

CASE IV.

Mrs. G—— was delivered on the 25th December, 1808, at six o'clock, P. M. after a labour of some hours, of her first child. The placenta was extracted in about fifteen minutes without force. There was some hæmorrhage, and considerable pain. She was put to bed, and became very faint, and complained of great pain, which was occasionally augmented. She continued in this way, only gradually becoming worse, until nine o'clock, at which time I was sent for.

I found her with a small frequent pulse, great anxiety, extremely pale and cadaverous, and in a profuse cold sweat. I inquired respecting the flooding; but this did not appear to be sufficient to account for her present situation. I immediately suspected a partial inversion of the uterus, and thought proper to apprize her friends of the probable cause of her distress and danger, and of the possible result of it. Every thing was left to my management. Upon applying my hand to the abdomen, I found the uterus sunk pretty low in the pelvis, and indented at its top. I immediately after examined per vaginam, and found my conjecture but too true.

The uterus was found inverted, and its fundus was just within the os externum. I was much alarmed for my patient, as three hours or rather more had elapsed between the time of her delivery and my being called; she was much exhausted, and in extreme agony. I quickly introduced my left hand into the vagina, and applied the back of my fingers firmly against the tumour, while I moderated its influence in carrying the

uterus directly up through the pelvis, by having a gentle pressure made upon the abdomen above it. The tumour soon began to yield, and in about two minutes the fundus was completely restored.

On the third day after, my patient complained of a severe pain in the right side just above the ilium, for which I bled her freely and purged her briskly. Nothing unpleasant supervened after this; she might be said to have had a good getting up.

REMARKS.

Three hours was lost in this case, from a belief that all the pain and anxiety was owing to after pains as they are termed; but when 120 drops of laudanum did not relieve her, the midwife became alarmed, and I was sent for. This patient would have been spared much distress had the disease been instantly known; and the risk of death prevented, had the uterus been quickly replaced. I say risk of death, for this there certainly was, as her symptoms were as alarming as possible; nor was there any ground from experience to hope for a reduction of the fundus, as so much time had been lost. This case I deem highly important, as it teaches us not to abandon our patient under these circumstances, and to attempt reduction at whatever time we may be called. We certainly cannot limit the time at which this attempt shall be rendered unsuccessful; this may in some instances happen before the period of three hours, or perhaps one; and it may be possible at even a later period. May not the disposition to syncope in this case, have retarded the contraction of the body and neck?

It was insisted on, in this case, that no unusual force was used to deliver the placenta; it separated from the uterus spon-

taneously, and was expelled without introducing a finger into the vagina.

Philadelphia, 30th January,
1809.

Observations on Cold. By "RADCLIFFE."

SIR,

IF the accompanying attempt to burlesque what I conceive to be the sophistical arguments respecting the stimulant nature of cold, should not partake of too much levity and trifling to comport with the plan of your Museum, you are welcome to insert it. I am aware that it can hardly be admissible, even when viewed as I intend it; but my opinion of the subject is such, from the arguments I have heard, that I believe something of this nature would place it in a fairer view than serious reasoning. I know some of our most respectable physiologists think differently, and I would wish to treat no one with disrespect; but we all know that some of the most absurd theories have been entertained by the greatest men.

I am, sir, with respect, &c.

Dr. J. R. Coxe.

—

"*Difficile est, non satiram scribere!*"

DR. COXE,

In consequence of the arguments I have heard advanced by some ingenious speculative gentlemen of the present day, and especially from a perusal of the communication of "*Arbuthnot*," in the fifth volume of your Museum, I have been induced to pay some attention to the theory which they have advo-

cated respecting the nature of *cold.* They contend that cold is a stimulating agent; and some with whom I have conversed, acknowledge a belief in the existence of *frigorific particles.* That such is the opinion of "Arbuthnot," I take for granted; because he will hardly deny that heat is a stimulus; and if he admit that cold is a mere abstraction of heat, it would involve a paradox unworthy of that gentleman, to suppose we could increase stimulation by subtracting stimulus. It would be a novel kind of arithmetic indeed, to contend that by subtracting we add! He must therefore assume, as a datum, the existence of the *matter of cold;* which matter, being applied to the living system, produces excitement.

Now I have to inform you, that the ingenious arguments and suggestions in favour of this theory have led me analogically to the discovery of other theories; or rather, I have extended this theory to other matters, which, I trust, when stated, will forcibly illustrate the doctrine of the stimulant nature of the matter of cold. As the phenomenon of cold has been explained by supposing an influx of frigorific matter, so I have supposed that *dryness* was caused by the presence of what I propose to call *siccitatiferous particles*, or *matter of drought;* and that *darkness*, in like manner, was the effect of a preponderance of *tenebrific matter.* The analogy between our theories must be very evident; and I hope will appear still more striking, when we have traced them further, and considered their similarity when applied to the phenomena they explain.

"Arbuthnot" has perspicuously stated the numerous cases in which cold acts as a stimulus in common with *other* stimulant matters; and I think the advocates for frigorific particles must see in his arguments what *they* would deem good proof of the existence of those particles. I know, indeed, it has been

said, that cold is proved to be a mere absence of the matter of heat, by the expansion of bodies when heated, and their contraction, or diminution in bulk when cold: but, on the other hand, it may be said we have a proof that the matter of cold exists, in the bursting of an egg-shell (and other containing vessels), when a copious influx of frigorific particles takes place: and again, in Wedgewood's pyrometer, we observe a diminution in size when the frigorific matter is expelled. (For further proofs, vide *Musschenbroek et al.*) So it has been generally supposed that *drought* was only an absence of moisture, as evinced by the swelling of substances impregnated with the latter, and their subsequent contraction upon its expulsion; but we all know that a cord, or rope, will shrink when impregnated with moisture, and expand again when saturated with the matter of drought.

"Arbuthnot" infers that cold is a stimulus, because it excites "sensation" and "motion," which are the acknowledged effects of a stimulating power: so we observe, that the "moving fibres" of a fish are powerfully excited by the presence of siccitatiferous particles; convulsions and death are the result of a complete application of them. That "sensation" is produced in this case, as well as "motion," will hardly be denied; and we have a proverb in corroboration of the opinion, which is applied to persons who are suffering great *uneasiness:* they are likened to "a fish out of water." A dry tongue and fauces also produce uneasiness; in fact, the *positive* effects of a superabundance of the matter of drought are numerous and well known.

The same observations apply to the *tenebrific particles.* A predominance of those particles cause a dilatation of the pupil, and consequently a "motion" of the fibres of the iris, which is

one of the characteristics of a stimulus. That this "motion" is the effect of stimulus, we have the analogical proof of the same result being produced by the effusion in hydrocephalus, and by the exhibition of certain medicines, which every one will admit to be stimulating! As to the "sensation" caused by an influx of tenebrific matter, every person must be conscious of it on going suddenly from a strong light into a chamber which is replete with that matter. In addition to this, we have the authority of one of the greatest poets, a poet who was long enveloped in "tenebrious gloom," and who speaks of "darkness *visible*," of course, producing "sensation!"

"Arbuthnot" says, "a hot or cold bath, or even immersion of the feet into hot or cold water, *equally* and instantly produce shivering." So, in like manner, is ophthalmia *equally* cured by the application of tenebrific matter, and by scarification, and collyria of sulphate of zinc, which are known to be stimulating! "*When two causes produce precisely the same effect, is it not perfectly just to conclude that both act in the same way, and therefore that both are stimulants or both sedatives?*"

Whether I have adhered to the "strict accuracy of logical reasoning" or not, must be left to the logician to decide; but I humbly presume, that the same logic which enables "Arbuthnot" to prove cold a stimulus, will bear me out in the inference that darkness and drought are also stimuli. As to the correctness of Cullen's definition of stimulus, I would beg leave to observe, that, if my theory be not admitted, I shall entertain doubts; because I believe I have shown that darkness will cause both "sensation" and "motion!" and I shall be induced to think, that if my arguments are called sophistical, the same epithet will be equally applicable to those in favour of the stimulant nature of cold. I believe both theories stand on the

same basis, supported by the same kind of arguments; and on the same foundation I leave them, to be supported or exploded, as the good sense of the scientific may determine.

RADCLIFFE.

January 23, 1809.

P. S. There is one point in which the application of cold to the living body may possibly not have been generally viewed, and in which it may probably produce a stimulant effect. We know that when a very cold substance is applied, there will be a rapid transition of caloric from the animal body to the substance in contact with it; and it is very possible that the rapid passage of caloric *out* of the body, may produce an effect similar to that which is caused by an *influx* of the same stimulant fluid. It has been said that frozen mercury produces the same effect on the living body as a hot iron; the only difference being in the opposite direction of the caloric, in the former case passing *out of*, in the latter *into*, the body. If this be a fact, why may we not suppose the same principle to hold good when applied in a less degree? If the efflux of caloric be very gradual, there will be little or no stimulation, it is true; but if it be copiously and rapidly drawn through a part, no matter in what direction, it must stimulate. I merely suggest this idea, not as being a new one, for I have heard it advanced, but because I believe it has not been sufficiently noticed in the discussions respecting the physiological agency of cold. R.

Account of the Influenza, as it prevailed in Waterford, Saratoga county, New York, in 1807. *By Dr.* JOHN STEARNS.

Waterford, Saratoga county, New York,
23d August, 1808.

DEAR SIR,

THE winter and spring of 1807 were marked by no epidemic or uncommon disorders. A greater variety of cases of typhus and phthisis pulmonalis was all that distinguished this from preceding years. These cases, however, had entirely subsided some months before the approach of the influenza. The first case of this epidemic appeared in this village on the 7th of August. In Albany, ten miles south, it had prevailed some days previous, and was introduced into this village by those who had been attending the courts then holden in that city. Mr. James, who was first affected, had taken lodgings in a family consisting of fifteen persons; some of these occupied stores in different parts of the village. Excepting those who had taken the influenza in Albany, the members of this family were first affected; and I think I could trace the first symptoms of disease in those who had been most familiar with the sick. About the 13th of August the disorder had become general, and continued to prevail till the 18th, when its decline had evidently commenced, and by the 30th not a single case remained in the village. In the country adjacent, however, it did not disappear until the 6th of September.

I observed that males were generally affected before the females, particularly where their occupations exposed them most to the external atmosphere. Those women who were

exclusively confined to their houses escaped. Children were less affected than adults, and negroes still less than children.

Its progress was nearly from south to north, and appeared to be accelerated by an intercourse with affected places. Although it did not wholly depend on this intercourse, yet it was evidently a mean to facilitate its diffusion.

That this epidemic was communicated by some peculiar change in the atmosphere, is an opinion that I have been hitherto inclined to embrace. But the facts respecting its progress, which I have this season observed, conduce to a belief that it depends on some specific contagion. How shall we otherwise account for its first appearance in the family where Mr. James boarded, and for its gradual diffusion from that source? Why was not its appearance in different parts of the village simultaneous, and not dependant, in some degree, on an intercourse with diseased persons? The place where it first appeared was a public house, and situated at the northern extremity of the village: had it been wafted by the atmosphere, we should have expected its first appearance in the southern part of the village: through this part there is no public road, and this was the last place affected. These facts at least excite a doubt, and, taken in connection with others of a similar import, may be decisive of the question, whether this epidemic is propagated by contagion?

The influenza was ushered in by the following premonitory symptoms: a listlessness and lassitude, succeeded by a pain in the head (particularly about the frontal sinuses), back, and bones, with a general soreness in the muscles. The throat was also affected with a soreness that gradually extended to the trachea and ramifications of the bronchia. The irritation pro-

duced by the inspired air excited a violent cough, which was the last symptom that disappeared. A sensation of general distress was induced, which the patients were unable to describe; the stomach was affected with nausea and oppression. Where sneezing was a common symptom, the disorder was mild, and had a sudden termination: the discharge in these cases was so acrid as to inflame the nose and upper lip. The tonsils in some were tumified and extremely sore; excruciating pains in one or both ears affected some; ophthalmias, in some cases, were severe, and extremely painful. The pulse was generally small and slow, with very little tension. I found but two cases that required bleeding. The remedies were generally emetics, cathartics, sudorifics, and diaphoretics *pro re nata*. The cough was successfully treated with a tincture of the balsam of Tolu.

It terminated in different affections of the lungs, but in no case fatally.

The two months immediately succeeding the termination of the influenza were remarkable for salubrity. The predisposition to disease appeared to have been destroyed by the preceding epidemic.

Yours, respectfully,

JOHN STEARNS.

Dr. J. R. Coxe.

Observations on Jaundice, read before the Philadelphia Medical Lyceum, November 16, 1808. *By* ELIJAH GRIFFITHS, *M. D.*

MR. PRESIDENT AND GENTLEMEN,

BEING about to offer some observations on jaundice, I shall pass by such causes as are considered local, and confine my remarks to those only that are general.

In the course of these remarks, dropsy will claim some attention, and, from the relation it bears to the former disorder, it will of course be introduced.

There is certainly no better way of arriving at a rational mode of curing disorders, than by investigating and understanding their causes. This is part of my present object. It is a fact well known to physicians, that these complaints are almost always preceded by fever, or by intemperance in the use of ardent spirits.

In a few cases jaundice may appear without previous fever, or irregularity; but, with professor Rush, I would ascribe it to the cause of disease passing by the arterial system, and attacking the liver directly.

It is highly probable the celebrated Darwin was correct, in ascribing one kind of jaundice to a torpor of the liver; we can readily conceive, that the vessels of this very vascular and delicate viscus may become paralyzed by the stimulus of overdistention, which will always be the case in fever left to itself, or attempted to be cured by heating nostrums.

Although I cannot hesitate to believe in the existence of the elements of bile in the blood, yet I am not prepared to admit, that a suspension of action in the liver is the only cause of its being rendered sensible, for blood drawn from the vein of a healthy person exhibits no bilious appearance at that time, or any subsequent period; further, the most extensive inflammations, and destructive suppurations, as well as schirrhi of the liver, have existed without producing true jaundice.

We shall proceed next to inquire whether vicarious functions are not performed by certain parts of the body, under peculiar circumstances.

A fluid very much resembling urine has been discharged from the stomach*, at the time of the kidneys labouring under a suspension of their functions from disease; and a discharge of blood from the stomach has been frequently witnessed, at the proper period when the menses should have appeared, but did not; the same vicarious discharge has occasionally come from the nose, and from other parts of the body, in suppressed catamenia†.

Admitting the above position to be correct, we can readily conceive of the extreme vessels of every secreting organ of the body departing from health, by taking on the function of the liver, and separating at least the colouring matter of bile from the blood.

The bilious tinge of the serum of the blood is still unaccounted for; but when we view the immense changes wrought

* Mem. Acad. Royale des Sciences, 1715, p. 12.

† Boerh. Acad. Lect. vol. 5, p. 115 and 118.

on the blood by the sanguiferous system, in inflammatory and malignant disorders, we will not be struck with so much surprize at this system being capable, by a disordered action, of rendering sensible the latent elements of bile contained in the sanguineous mass.

We shall, therefore, consider the yellow tinge of the patient, the torpid bowels, and dyspeptic state of the stomach, as links in the chain of symptoms denoting a disordered state of the body, either dependent on the original cause, or on the torpor of the liver.

When, in these cases, the absorbents have become very inactive, and the secerning vessels of the arteries have assumed the nearly suspended functions of the kidneys and perspiratory vessels of the skin, we may look for dropsy to follow, with the utmost certainty.

It would be superfluous, in this place, to offer any arguments to prove that fevers and intemperance are the most common causes of jaundice and dropsy. We shall therefore proceed to inquire how they operate as causes.

It has been long known that stimulating remedies, or even a neglect of depletion in fevers, have produced these complaints. The great declension of the solids, and corresponding increase of the fluids, in fevers that have lasted some time, prove incontestibly the sanguifying power of the absorbent system. But the absorbents are not capable of taking up bones, flesh, or even fat, undissolved. We must then look for a solvent of these solids in an inversion of that power which originally formed them from the animal fluids.

What that power is, or on what actions of the animal economy the consolidation and solution of living matter depends, I confess my ignorance; however, we are sensible of the effects of this process in fevers, by the rapid increase of fulness in the sanguiferous system, so as to effect the changes above named. Hence the efforts of nature to relieve herself by hæmorrhagy, profuse sweats, diarrhœa, &c. which also prevent many dangerous terminations of fevers, unsubdued by the hand of art.

The due oxygenation of the blood is known to be essential to the healthy actions of the animal economy. This can hardly be expected to take place, when the transmission of blood through the lungs does not bear a proper ratio to the whole mass; for in the stage of undisturbed fever, tending toward a chronic state, we find the pulse quick and frequent, but small: now it is fair to conclude a correspondent action takes place in the heart and pulmonary arteries; an engorgement of the viscera, with deficiency of oxygenation, will be the consequence, and must have a most extensive influence on the vital functions of the body, in altering the appearance and quality of the fluids, diminishing the secretions, favouring effusions, and retarding the action of the absorbents, by choaking up the outlets of the thoracic duct.

By way of illustration, I beg leave to offer the following few cases, regretting that I have not had a more ample opportunity of deciding on the efficacy of the means, previous to laying them before the society.

In March, 1807, I visited Mr. M. who had for a considerable time laboured under a quotidian form of fever, for the cure of which he had made a very liberal use of Peruvian bark. This had diminished the violence of the paroxysms, but greatly

protracted their duration, so as to give the disorder nearly a continued form of fever, with a periodical sense of cold, but not a chill. The pulse was weak, but frequent and quick; he was free from pain, but very weak. Tart. antimon. joined with sulph. sodæ, were given to the extent of full vomiting; a blister was applied over the stomach; after which nitre, tart. antimon. and calomel were given till the mouth was affected.

The fever now assumed the quotidian form, with fair paroxysms and intermissions, and was to all appearance speedily cured, by the combined use of Peruvian bark and soda.

After a few weeks, I again visited this patient, and learned he had continued weak, with loss of appetite and constipated bowels, ever since I had left him. His skin was considerably tinged by bile, alvine evacuations of a greyish colour, bowels slow, pulse small and weak; he was free from any pain or uneasy sensation in the region of the liver. Blood-letting and calomel were prescribed; the bleeding was not done; the calomel produced no other effect than moving the bowels.

A variety of medicines were resorted to without any good effect, for the disorder daily grew worse.

With this state of things Mr. M. become discouraged, and had recourse to empiricism, which was tortured for a time, without any change, except the patient's being worse than when he began. Blood-letting was now used for the first time, with the most happy effect, for in less than twenty-four hours bile was discovered in the evacuations from the bowels; another bleeding sensibly diminished the yellowness of the skin, and one or two more removed the disorder, without any other remedy.

An indented servant man, belonging to Mr. Kraft, near Kensington bridge, was taken down with a violent bilious fever, in August, 1807. This patient sunk so much under the disorder that the lancet was but sparingly used. His convalescence was very slow, with slight occasional fever, debility, and very little appetite. This was soon followed by a tinge of yellow on the skin, and ash-coloured stools. A strong decoction of the Barbary bark was given freely, but the yellowness increased to the deepest tinge. I now had recourse to blood-letting, although the pulse was weak; two bleedings and a purge of calomel presented bilious stools. The yellowness soon disappeared, and the patient recovered perfectly without any other remedy.

Mr. F. W. a bricklayer, about thirty years of age, in January, 1807, complained of a pain in his side, with laborious respiration, which was much increased by a horizontal position; at the same time his abdomen was hard, and very much swelled.

He was advised, by some friend, to take a strong decoction of digitalis, which operated as an emetic and cathartic, and speedily removed the complaint. He remained well till about the end of June, 1807, when he became affected by a tertian fever, with very indistinct paroxysms.

He had medical advice at this time, but was not relieved by it; the difficulty of breathing returned, with swelled legs, and a hard tumour in the epigastric region. Application was then made to a notorious German impostor, who professed to cure all disorders by vegetable teas. This king of quacks amused him for some time with hopes of relief, but finally left him far worse than he found him.

On the 27th of November, 1807, I first visited Mr. W. and found his pulse very frequent, weak, and threadlike; his extremities cold, and very much swelled; respiration so laborious as to oblige him to sit erect night and day; his abdomen contained a large quantity of fluid, and a large tumour could be traced from under the scrobiculus cordis, below the umbilicus, and a little to the right side.

The urine was in small quantity, and very high coloured; bowels slow, appetite bad, with occasional nausea, and a dull pain about the stomach.

Calomel, nitre, and tart. antimon. were tried in small doses, but disagreed with his stomach so much that they were changed for a weak decoction of digitalis, with carbonas sodæ, which was also immediately rejected by the stomach.

After trying every formula of medicine I could devise, and each proving equally offensive to the stomach, and the system sinking under the disease, with a pulse scarcely to be perceived, and too frequent to be counted; cold, clammy sweats, and the breathing so obstructed as to give the skin a livid hue, I determined to unload the system by puncturing the legs and feet. These punctures discharged very freely, and gave considerable relief to the breast. A blister was applied over the stomach, and dressed with mercurial ointment, also ʒi of vitriolic ether was given every two hours. On my next visit I was astonished to find his pulse at sixty strokes in a minute, regular, somewhat full, but rather corded. The stomach was so well as to retain drinks, medicines, &c. The respiration was much less laborious, and the anasarcous swelling greatly diminished by the free evacuations from the punctures.

The ether was continued, but in smaller doses.

Blisters were applied to the legs; they, and the punctures, soon assumed a livid appearance, and threatened mortification; the yeast poultice reduced the inflammation, in two or three days, to the most simply suppurative kind; the patient had been for the last three days taking sp. nit. dulc. in parsley tea, which proved very diuretic. The dropsy was now entirely removed, and the patient approached nearer the figure of a skeleton enveloped in skin, than that of a man in ordinary health.

The pulse was small, but frequent and very tense; the excretions were in very diminished quantity and slow, unless urged by medicine. A yellowness began now to appear on the skin, with such excretions as are common in jaundice. A strong decoction of the berbaris vulgaris was administered, and continued for some days, without any benefit to the patient. The pulse continued small, corded, and about 90 strokes in a minute; the yellowness of the skin had arrived at the deepest shade.

From the good effects of blood-letting in the two former cases, I was induced to try it in this. The first bleeding produced no change, but the second did in the colour of the skin, and excretions; the third and fourth bleedings removed most of the yellowness and other symptoms, except a most incessant cough, which had recently come on, and soon disappeared by a copious expectoration. By the free use of mercurial ointment to the skin, and blisters, a gentle salivation was induced. The tumour in the abdomen disappeared, with every other appearance of disease but debility; when a course of gentle tonics was advised.

I now concluded my patient was quite well, but in that I was mistaken; for, on the 5th of January, 1808, Mr. W. complained of a pain in the region of the liver, head-ach, and some oppression at the breast; his feet and legs began to swell considerably, and his pulse was more than usually corded. I drew ℥x of blood from his arm, and directed sp. nit. dulc. in parsley tea. On the 6th and 7th, his pulse was very irregular; he was bled each day. By examining the abdomen, I found the spleen hard, and very much enlarged; a blister was applied over it, and dressed with mercurial ointment. This had a very good effect in dispersing the tumour.

Between this and the 15th, several diuretic medicines were taken, but without any benefit; the pulse was generally about 95 strokes in a minute, small, and a little tense. The 15th January I examined his abdomen, and found his liver very much enlarged and hard; nitric acid was administered very freely, and continued till the 19th instant. The pulse was 110 in a minute, with an aggravation of all his symptoms; ℞. pulv. fol. digital. gr. ij cal. ppt. gr. iss three times a day. 23d inst. his mouth was sore, pulse 100 and a little corded, but fuller; tumour in the abdomen diminished in size, and the other symptoms all abated.

From this till the 31st, he took his medicine in less quantity, on account of sick stomach; I found the salivation alternate with severe pains of the bowels several times.

His pulse was reduced between the 19th and 31st, from 110 to 60 strokes in a minute. February 2d, the pulse was at 40 strokes in a minute, and regular. The medicine was omitted till the 4th, when the pulse had risen to 60. He was directed

to take one powder every night. The pulse rose to 120 the 11th, but sunk to 84 strokes by the 16th. Every symptom of indisposition had disappeared; the pulse was soft, regular, and fuller than at any previous period.

Mr. W. continued to mend from this time, and I had the satisfaction to see him in the enjoyment of perfect health last August.

I have been thus particular with the last case, in order to show how low a patient may sink in these complaints, and still be restored to health.

Woodhouse's Reply to Seybert's Strictures on his Essay concerning the Perkiomen Zinc Mine.

To the Editor of the Philadelphia Medical Museum.

SIR,

IN a work dedicated to the interests of science, it ought certainly to be expected that those whose leisure or opportunities permit them, occasionally, to throw in their contributions to the general stock of knowledge, would discard every thing like *asperity* in their remarks on the opinions advanced by others; and that the *little* passions of *envy* and *jealousy* would never actuate the minds of those, whose *real* object is the pursuit of truth.

It was, therefore, with surprize, mingled with regret, that I perused the paper inserted in your last number by Dr. Seybert, on the subject of the Perkiomen mine.

As it is of little consequence to the public whether or not Dr. S. knew that *blende* or the *sulphuret of zinc* was found near Perkiomen in 1806; and, as it is equally immaterial, whether or not, in 1807, when shown a specimen of this same ore, he declared it to be *lead* ore; I shall proceed to show that his essay, improperly entitled "Facts [when it entirely consists of quotations] to prove that this metallic ore can be worked to advantage in the United States," proves nothing, except the doctor's misplaced rancour against myself, and which my former essay has furnished him a pretext for exhibiting.

Without entering into a comparison of the doctor's patriotism with my own; without pretending that my attachment to my *natale solum* is as strong as his; or that I should be disposed to make as great sacrifices, either personal or pecuniary, for my native country as Dr. Seybert would, I shall show,

1st, That there is an evident *want of candour* in the conclusions he has drawn from my publication.

2d, That some of his quotations from chemical writers are unfairly given.

3d, That what he has advanced bears no direct relation to the subject in question.

4th, That the observations in his concluding paragraphs are *highly personal* and improper; and

Lastly, I shall annex *correct* extracts from the best modern writers to show that the *blendes*, though they abound, are seldom worked in Europe.

In the first place there is a want of candour, because the doctor asserts that I have maintained that this American ore will yield 72 per cent. of metal; whereas I expressly mentioned that this quantity was *not* given as *accurate*, from the difficulty of analyzing the ores of *zinc*, and the reasons are assigned.

An erroneous conclusion is drawn from what I have published on this subject. In order to place this in a clear point of view, I will here insert the two paragraphs which have so much excited the irascibility of the doctor, and let the reader compare them with his remarks.

"Can this ore be worked to advantage in the United States?

"No information on this subject can be obtained from any book with which I am acquainted. Mr. Meade, a gentleman possessed of extensive knowledge on mineralogy, informed me that it is never worked in England. Dr. Bruce, professor of this science in the College of Physicians, New York, told me that it is reduced in Wales; and Mr. Godon, of Boston, who is extremely well acquainted with subjects relating to this business, has declared that the zinc cannot be obtained from this kind of ore, but with the utmost difficulty."

For thus merely stating the information derived from three men of eminence, without advancing any opinion of my own, Dr. Seybert has taken the liberty of asserting that I assume the principle that blende is not and cannot be worked *any where* to advantage.

Secondly, The quotations from some writers are not fairly stated. This will appear from the following extract from Dr. Seybert's essay, when compared with what has been said by bishop Watson and the celebrated Chaptal, on the same subject.

"At Rammelsberg, near Goslar, there is a considerable manufacture of brass. I visited it in 1794. Here they form this important alloy with (cadmia) a sublimed oxide of zinc which is obtained by proper management during the roasting of the lead ores and blendes, in a reverberatory furnace."

Here an incorrect idea is held forth, that a mine of *blende* is worked near Goslar in Germany, but the fact is, that the mine at that place is wrought for the *lead* and *silver* it affords; and, as no additional expence is incurred from the purchase of fuel, the oxide of zinc is obtained at the same time; and the mine is a *lead*, and not a *zinc mine*.

Now let us examine what Chaptal and bishop Watson actually do say on the subject, when divested of the doctor's *alloy*.

Chaptal, vol. 2, p. 46, 4th Amer. edit. "Zinc is sometimes mixed with lead, and in the working of this last metal, the former is *occasionally* obtained. Such is the ore worked at Rammelsberg, near Goslar. Great part of the zinc is dissipated, but a *portion* of this metal is obtained by a very ingenious process."

Watson's Chemical Essays, vol. 4, p. 40. "At Goslar, in Germany, they smelt an ore which contains lead, silver, copper, iron, and zinc, in the same mass. The ore is smelted to

procure the *silver* and *lead;* but, by a particular contrivance, they obtain a portion of zinc in substance."

The slightest observation will show that Dr. S. has taken as much from bishop Watson's work, as would suit his purpose, without any regard to conveying the true meaning of the author.

It is true, the bishop says, vol. 4, p. 20, "The sulphuret of zinc has for many years been used, as well as calamine, for the making of brass at Bristol;" but, in p. 40 of the same essay, he informs us, that "as to this ore of zinc, it is not so commonly used as calamine, for the making of brass at Bristol. Several ship-loads of it were sent, a few years ago, from Cornwall to this town. Upon the whole, however, experience has not brought it into reputation at this place."

But Dr. S. infers, that because a lead mine in Germany, which is worked for the silver and lead it contains, affords a *portion* of zinc, that therefore a zinc mine, containing little lead and no silver, can be worked to advantage in the United States.

Thirdly, What the doctor has advanced in near eight octavo pages, bears no direct relation to the subject in question. It is not by quotations from foreign writers that we can determine whether metallic zinc, or any substance into which it enters as a component part, can be worked with advantage in the United States: but by taking into consideration the extent of the ore; the quantity of metal it will afford when worked on a large scale (which can only be ascertained by an experiment with several hundred weight of the ore); the price of labour in this country; the cost of fuel to throw off the sulphur, and

afterwards to extract the metal; the demand for the zinc, and the price for which it can be manufactured abroad and imported into this country. Not one of these circumstances have been considered by Dr. Seybert; and yet, with a kind of self-complacency, as if the consideration of these points was beneath his dignity, he says, "I DO MAINTAIN that the Perkiomen blende can be worked in America with advantage." There is a trifling difference, however, between *assertion* and *proof*.

The question is not whether the blendes are, or are not, worked in *Europe?* but, whether we can manufacture metallic zinc, or compounds into which it enters, cheaper than they can be imported from England, France, Germany, and the East Indies.

Calamine, an ore of zinc, which can be easily and profitably wrought, abounds in Great Britain. "As we have greater plenty of calamine in England," says bishop Watson, "and that of the better sort, than most other nations have, there is no fear of our losing the advantage in this article of trade, which we are now possessed of." *Essays, vol.* 4, *p.* 7.

The blendes of Perkiomen also differ, *materially*, from those of other countries; and they all differ from each other.

"The nature of the sulphurets of zinc," says *Fourcroy, vol.* 5, *p.* 519, "is not well known, and Bergmann found such great differences between them, from different countries, that the ores seemed to possess no identical properties, or composition."

Fourthly, The observations in the concluding paragraphs are highly personal* and improper. Although I had advanced no opinion, the doctor has undertaken to *denounce* me as one "exciting unfounded doubts, propagating erroneous opinions, and *attempting to paralize* the wise efforts of a judicious public," (although no efforts have ever been made by the public, to work the Perkiomen mine), "and at a time when the political situation of our country is such, that its foreign relations are interrupted, and much is expected from an energetic application of its internal resources."

In France, during the gloomy periods of the revolution, such a denunciation would have been sufficient to bring the object of it to the *guillotine;* and, *were it true*, it ought, even here, justly to excite the hatred of my fellow-citizens against me. But I repel the insinuation with the contempt it deserves. In every analysis I have made, public utility has been my sole object, and to that object my attention has always been cheerfully devoted, without any regard to labour or expence.

Correct extracts from the most celebrated modern writers are annexed, which prove that the blendes or sulphurets of zinc are *seldom* worked in Europe.

The abbé Hauy, whom Fourcroy, the great and enlightened historian of chemical science, very justly styles "the last, most

* The Editor in justification of himself must remark, that however the paragraphs may have appeared to Dr. W. to himself they conveyed no marks of personality, which would have precluded the admission of the communication. He trusts that no further remarks, which can in the remotest way be viewed as personal, will be offered for insertion, as it is the furthest from his wishes to encourage any thing of the kind, although a fair and candid discussion of any subject will always meet his ready attention.

learned and accurate author on mineralogy," after mentioning that the sulphurets of zinc abound in the mines of Saxony, Bohemia, Hungary; that they are found in Sweden, Norway, England, France, &c. and that, in general, zinc is one of the most common of the metals, informs us, that "this metallic substance is scarcely an object to seek after, and that it is casually extracted in the melting of minerals with which it is associated, and particularly the sulphurets of lead*."

"The sulphurets of zinc are scarcely worked by themselves, or with the sole intention of extracting the metal. It is most frequently by fusion with the ores of lead, mixed with sulphuret of zinc, that the latter metal is obtained." *Fourcroy, vol. 5, p. 522.*

"Blende is sometimes, although extremely rarely, worked as an ore of zinc." *Jamieson's Mineralogy, vol. 2. article Zinc.*

"Zinc is obtained *adventitiously*, in the melting of such copper and lead ores as contain zinc or blende." *Weigleb, p. 419.*

"*I do not know any country where blende is wrought to obtain zinc.*" Chaptal, Amer. edit. vol. 2, p. 46.

"As the consumption of zinc is very limited, it has not been much worked." *Chemistry applied to the Arts and Manufactures, by Chaptal, vol. 2, p. 209.*

* "Cette substance métallique n'est guère un objet direct d'exploitation. On l'extrait accessoirement par la fonte des mines auxquelles elle est associée, et en particulier du plomb sulphuré."

Mineralogie, par Hauy, tom. 4, p. 170.

"Calamine is the ore of zinc that is always worked. The extraction of zinc from blende is attempted, but not often." *Murray, vol. 3, p. 34.*

"*No mines are worked in order to extract zinc.* In fusing lead ore, mixed with blende, the metal is obtained in the state of an oxide." *Lagrange, vol. 2, p. 34.*

I might swell this list of quotations with extracts from other eminent chemical authors; but I shall forbear, as I should, no doubt, meet with this *conclusive* answer from Dr. *Seybert*, that "*Those authors do not know all that is done in this way!**"

The reason that the blendes are not worked, is the great difficulty of throwing off the sulphur they contain. The following experiments have lately been made:

One pound of the Perkiomen ore, reduced to a fine powder, was exposed eight hours in a blast-furnace to the heat of the Lehigh coal, which is much greater than can be excited by any other kind of fuel, and it lost only three ounces in weight. Aqua regia was then added to an ounce of it, and a quantity of sulphur immediately separated and swam on the surface of the fluid. When washed and dried it weighed 150 grains.

Eight ounces of this roasted ore, mixed with the powder of charcoal, were submitted in a proper manner to the same heat for ten hours, and an inconsiderable quantity of metallic zinc was procured.

An unsuccessful attempt was also made to manufacture the sulphate of zinc, or white vitriol, in the same manner as they

* Seybert, p. 211, Medical Museum, vol. V, No. 4.

do in some parts of Europe, by submitting a lump of the ore, weighing two pounds, to an intense red-heat, repeatedly extinguishing it in water, and evaporating this fluid to dryness.

As Dr. S. has made no experiments on the Perkiomen ore, it is absurd for him to pretend to give information to others on this subject, when he possesses none himself.

For my part, I would sincerely rejoice to see this or any other metallic substance wrought *to advantage* in this country: coinciding freely in the opinion of the illustrious Chaptal, "that although AGRICULTURE is the basis of the public welfare, the ARTS and COMMERCE form the *glory*, the *ornament*, and the *riches* of every polished nation."

I shall now conclude my reply to Dr. Seybert by introducing a letter on this subject, which has been addressed to me by a mineralogist, who, in that science, is second to none, either in this or any other country.

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SIR,

THE question at present between you and Dr. Seybert is, whether it be possible to use, *with advantage*, the blende of Perkiomen in the manufacture of zinc and brass. It appears to me that the solution of this question is beyond the limits of chemistry and mineralogy, and becomes a question merely of speculation. On this point, I think the quotations from European authors perfectly useless, for none of them declare positively that any benefit has resulted to those who have tried this experiment. Besides, their authority must pass for nothing as it regards America, where circumstances are so dif-

ferent from those of Europe. I have no hesitation in giving my opinion against a manufacture intended for the transformation of *blende* into *artificial calamine*, in a country where I am as yet unacquainted with any mine of copper being in *actual exploration:* but my idea on this subject may be susceptible of some modification, as I am not sufficiently advanced in a knowledge of the country, to have ascertained the price of various articles necessary to the manufacture, and to render it really serviceable.

Dr. S. anticipates the near approach of a time, when we shall see the articles of zinc and copper, as forming interesting items in the list of articles exported from the United States. If this should prove correct as to *brass*, it certainly never can be so as to *zinc*. I have always seen that metal at so low a price, and in so little demand, in Europe, that it is very doubtful whether that part of the world (the only place to export to) will ever present an advantageous market for zinc *manufactured in America*. But if, in fact, Dr. Seybert has a good opinion of the utility of exploring this mine, a natural *proof* presents itself for corroborating his own opinion, and proving that what he advances to the public is the *fruit of reflection*. It is said he is wealthy: let him furnish the funds, and his information, towards an object of which he has so high an idea, and which ought now to afford a greater prospect of success, as a mine of copper, of which *he had no knowledge at the time of publishing his essay*, has just been discovered near that containing this blende. For my part, I sincerely hope this business may succeed; but so far I see no reason to change my opinion.

Observations intended as a support to the Doctrine of Cuticular Absorption, and in opposition to opinions of late advanced on this subject. Read before the Medical Lyceum, February 22d, 1809.

IT is not my intention to enter largely into the subject of absorption, nor to consider the question still proposed by the Lyceum as a fit subject for a premium, whether "the human body possesses the power of absorbing substances applied to its surface?"

It will readily be admitted that it is easier to object to an established hypothesis, than to raise another free from all objection, in its place. Yet, however I may estimate the very ingenious experiments of the different authors on this subject, I cannot but say that my mind is not converted to their mode of thinking; nor indeed am I able to give up to their forcible arguments the plain evidence of my senses.

My chief inducement, in the present instance, is to oppose a doctrine which I perceive is upheld by the opponents to cuticular absorption, viz. that this function is carried on by the lungs, which are *supposed* to receive the volatile emanations through the medium of respiration; the facts and arguments to prove which, allowing every force which can be demanded against cuticular absorption, appear to me so vague that I cannot withhold my dissent from it. Should future experiments demonstrate fully that the skin is not the grand superficies whence absorption takes place, I must still dissent from allowing the lungs more than a partial right to it, if indeed they come in for any share whatever of the function.

I shall here observe that the opponents to cuticular absorption admit this function taking place, provided the cuticle, that seven-fold shield, be removed; but they leave us totally in the dark as to the use of the numerous lymphatics which are spread over the surface of the body. Now, as nature certainly never intended the removal of the cuticle in a healthy state; and, as it is clear that so large a congeries of vessels could never be proposed for no use, I think we are warranted in demanding of the gentlemen, what purpose they are intended to answer?

It appears to me, that from the general tenor of the experiments, and the substances employed, the gentlemen consider the lymphatics as solely an appendage to the urinary bladder, as we find them almost constantly attending to that viscus, to discover in the urine the presence of the articles employed, whether those experiments be intended to disprove absorption from the surface, or to assert the dependance of this function upon the lungs. It is true, that they are presumed (at least I suppose this is the presumption) to convey their contents thither through the medium of the blood-vessels; but can we not safely suppose, that however the presence of some articles, as turpentine, may be detected in the urine, there are others which are conveyed by the same channels to other emunctories of the body, without having any aptitude to be discharged by the bladder? If so, the absence of any particular substance in the urine certainly cannot disprove its being conveyed by absorption from the surface.

It is very surprizing to me, that with the absolute certainty of absorption going on in the stomach and intestines, these parts have never had, at least, a portion of that power ascribed to the lungs assigned to them. Certainly their right in an

equal, if not superior degree, cannot be denied: but it would seem that the opponents to cuticular absorption, so long admitted, appear to think, unless they point out an absolutely new channel for this function, to the total exclusion of the skin and alimentary canal, they do nothing. But it will not be denied me, that the stomach and intestines* possess every power as it regards this function, which they so sedulously ascribe to the lungs solely. Every experiment they detail, proves this sufficiently; for it is not possible to convey any thing into the lungs, a part of which, inviscated in the saliva, shall not descend into the stomach. If then, I say, their experiments disprove, *in toto*, the absorption from the surface, they must at least admit the stomach and intestines to an equal share with the lungs. But, as I consider it as a more just doctrine, I must regard the former as possessing entirely this function, if cuticular absorption be at any time overturned.

In opposition to that doctrine which dissents from cuticular absorption, I shall remark, that, in my opinion, experiments founded on the external application of spirit of turpentine are very ill adapted to disprove the point aimed at. Without giving to the lymphatics the power of election, it appears very unlikely that these minute vessels could in any case be induced to take up an agent so acrid and stimulating, that we know of

* After all, the stomach, &c. are still a part of the exterior of the body, and differ only from that generally so esteemed, by the coating of cuticle; the action of this amazing congeries of vessels being unique, whether on the external or internal surface, or within the cavities of the body. Must we then actually suppose, that the mere appendage of cuticle overpowers the function appertaining to the lymphatics! When we see the function of perspiration still provided for in spite of a cuticle, would it not better become us to presume, that, by some hitherto unexplained contrivance, the cuticle is in fact no more an opponent to the entry than to the exit of fluids?

none better adapted to constringe vessels to which it is applied; and hence its great utility in stopping hemorrhagies. Now, if vessels so much larger are coerced to close upon its application, even when aided by a discharge of their fluids, it surely requires little reflection to perceive, that on the more minute and delicate fibres of the lymphatics, this effect must be still more powerful. Hence then, I say, these and similar experiments can never be adduced against cuticular absorption with any success; and, although the same arguments may be urged against the lymphatics of the stomach and intestines, equally with those of the lungs, although fact evinces its presence in the urine after inhalation, yet I must contend that there is a wide difference between the palpable application of the substance itself to the external surface, and that which arises from its mere volatile and finest parts inviscated in the saliva, and carried into the stomach*.

It may be said that the effect perceptible in the urine is too rapid to explain it by this circuitous route; but, does not the same objection, in a degree, hold good as it applies to the pulmonic theory of absorption? But the experiments of Mr. Home, lately published, give us a very ready explanation of this rapid passage to the bladder, and confirm the doctrine I have advanced, and which I have many years upheld, al-

* One of the first and most indefatigable opponents to cuticular absorption, in denying the conveyance of simple fluids into the system, through this channel, ascribes the prevention or cessation of thirst, by the application of wet cloths to the skin, to the constriction of its pores, by which perspiration is diminished. If so bland a fluid as water could not gain admittance, surely experiments by a fluid so much more acrid, might be deemed useless. But if the theory be just, whence the thirst in fever, when perspiration is so greatly obstructed? and whence the immediate relief from thirst, when a free and profuse perspiration is by any means excited?

though it has never yet, that I know of, been adverted to by the graduates of this university, in their inaugural dissertations upon this subject.

In referring to the experiments detailed in the essays I have read on the subject, I find that the turpentine, in every case when the violet smell was perceptible in the urine, was inhaled, or in other words was carried both into the stomach and lungs (for I consider it as absolutely impossible that some portion should not be carried into the former viscus entangled in the saliva); whilst, on the other hand, when the violet smell was not sensible, there an exclusion took place equally from both organs. Hence, the gentlemen cannot urge an objection against the absorption of the one, which is not equally conclusive against the other; nor advance a fact which will not be as favourable for my opinion as for theirs.

I have said that Mr. Home's experiments very satisfactorily explain, and without the aid of Dr. Darwin's retrograde absorbents, the rapid manner in which fluids, &c. are conveyed from the stomach to the bladder of urine. This celebrated anatomist found that the fluids are principally contained in the cardiac, or left portion of the stomach; whilst the more solid contents are consigned for digestion to the pyloric extremity; the fluids, above what are accessary to digestion, being carried out from the stomach, without ever reaching as far as the pylorus. A series of experiments convinced him this was effected by vessels passing from the stomach to the spleen. In some of these experiments, the effects of articles taken into the stomach showed themselves in the urine, within a quarter of an hour after being taken (the period at which the most early detection of spirit of turpentine has been made in that fluid).

Another fact, confirming the first, and explanatory of a circumstance I was until now unable to account for, is, that after some time the effect on the urine was no longer evident; but at a more distant period of nearly seven hours, it became again sensible in the contents of the bladder.

Here then we see a portion of the substance employed (rhubarb) rapidly passing off with the fluids from the stomach, and exciting evidence of existence in the urine, by the route through the spleen; and at a later period, when we may suppose another portion had passed with the chyle into the intestines, and thus, through the more distant course of the lacteals, again evincing its presence in the bladder.

The fact I allude to, as now explained, is that I have repeatedly detected, on eating asparagus, the peculiar unpleasant odour it produces in the urine, in a very short period after my meal. This, in a few hours, entirely subsided, but recurred after a longer interval, in which that portion not taken up immediately from the stomach to pursue the route by the spleen, followed the more circuitous one through the pylorus and lacteals.

By these experiments we can now readily explain the rapid excitement of urine on eating water-melon, and the frequent call for micturition when drinking freely after dinner, or on taking punch, or other articles favouring the secretion of that fluid. We see also a reason for the quick subsidence of those unpleasant feelings excited in the stomach by a copious meal, and which certainly the stomach could not, without hazard, contain entire, until carried off by the slow operation of digestion; if indeed digestion could be at all performed when so large a mass had over-distended the stomach, and probably, like

the over-distended bladder, to a certain degree paralized its powers of action. Fearfully, indeed, and wonderfully are we made, when such various channels of preservation from destruction so clearly point out the superlative wisdom of a Divine Architect.

To the objections made by the opponents to cuticular absorption, to the introduction of mercurial ointment, by the external lymphatics, I can only say, that the opposition to a fact, or the failure of detecting the presence of that substance in the fluids, can only be regarded as negative, not positive proof. What changes may be induced by the action of living matter, either in a mechanical or chemical view, so as to prevent its detection by common tests, I know not. But, until I see stronger reason to doubt the evidence of my senses, than these gentlemen advance against its absorption, I must be allowed to deny my faith to the extraordinary manner by which they suppose it to be introduced into the system, viz. by being carried in a state of volatilization into the lungs. I must confess I regard this opinion as savouring too much of the straining at a gnat and swallowing a camel. I certainly may be allowed to demand of them their proof of this assumed fact. Is it not a mere petitio principii, strangely urged to favour their hypothesis? "Give me a place to stand on," says Archimedes, "and I will move the world." So they, "Grant me this, and I will readily prove the other." But assertions are not proof, and proof I ask, that mercurial ointment is ever volatilized by the degree of heat excited by merely rubbing it on the surface. By exposure of ℥i of strong mercurial ointment for two hours and a half to a temperature of 120° (a degree of heat, I apprehend, seldom exceeded by the friction of the hand on the skin), I found it lost only 15 grains, arising most probably from a loss of the sebaceous matter, and not of the metal. Again, let

these gentlemen, after guarding both hands with tight bladders, endeavour to produce the volatilization of the ointment, and tell us at the end of the experiment how much loss has been actually sustained in the ointment, after deducting the weight acquired in the process by the bladders themselves. I suspect the result will differ much from the same process carried on upon a naked limb. How is it also that we safely employ mercurial frictions for another, in large quantities, when guarded with a bladder, which shall nevertheless excite ptyalism in the patient, if the effect was derived solely from its volatility? Ought not *both* to be equally affected? I know the reverse has been stated, but certainly one solitary negative fact is not to overturn the host of proof in the opposite scale. By requiring too much, we often fail in establishing what we desire. If it is not carried in by the cuticular absorbents, it certainly is not by the way pointed out by these gentlemen; and my intention here is to attempt to overturn the foundation they have lain for a new superstructure in the system of absorption.

I would here recommend the favourers of this volatile theory to put it to the test, by suspending a piece of gold directly over the limb on which the ointment is rubbed, or in the centre of the venereal ward in the hospital, and inform us how long it actually takes to induce any appearance of amalgamation on it, visible by the volatilization of the mercury.

I shall here mention my opinion, that I think the smell of the turpentine in the breath, no more proves that it is taken in by that channel, than the smell of violets in the urine proves it to be absorbed by that viscus. The same vessels which convey the odorous particles from the stomach into the blood-vessels, give those blood-vessels, by the medium of the circulation, an equal chance of conveying them to the bladder and to the

lungs, and this is in a great degree proved to be the case, in some experiments detailed, where injections of turpentine were employed, and where its presence was soon after detected both in the urine and in the breath. Now certainly, as inhalation was particularly guarded against, I think all chance of absorption was completely cut off from the lungs, and its presence there could arise only from absorption in the alimentary canal, and conveyance to the lungs by the medium of the blood-vessels.

If I am here questioned relative to the various other articles employed in the experiments, I can only say that I have not yet fully considered, and must therefore be excused from a present reply. I have taken the two substances principally adverted to, and think I have fully established the point that turpentine never is, nor can be taken up by the cuticular absorbents, whose action nevertheless I consider as absolute; and that mercurial ointment never is, nor can be volatilized by the temperature employed upon its use; consequently, that as it disappears, it is actually taken up by the cuticular absorbents. I think I have also proved that the stomach, and not the lungs, is the viscus to which we are to ascribe the introduction into the system of those articles which nature never proposed, from their acrimony, to find a passage through the delicate superficial lymphatics, but which, by impregnating with their minute and volatile particles the food and saliva taken into the stomach, are thus enabled to evince their presence in very different and distant parts of the body. I have, therefore, if cuticular absorption is not admitted, at least done something in preparing the road for further inquiry.

Philadelphia, February 18th, 1809.

Observations on Anomalous and Irregular Diseases.

By Dr. G. WILLIAMSON.

Baltimore, November 2, 1808.

AMONGST the many interesting medical essays which are presented us through the medium of the various periodical publications, we are rarely favoured with any account of those irregular diseases which all physicians of extensive practice must occasionally meet with. These diseases, when violent, are generally very perplexing; to the young practitioner they are inexpressibly so: to him they are new, and he is at a loss with what order, genera, or species of disease to class them; and, should he have even thrown off the shackles of nosologia, they are still very perplexing. At one visit the disease appears in one form, at the next in another. At one time one viscus seems principally affected, and at another some other. Diseases of this nature cannot be reduced to any systematic order; the doctrines of diagnosis and prognosis are here equally futile. Brown's system is too simple, and Darwin's too abstruse; each would admit them within their classifications, but neither would enable us, in many cases, to speak intelligibly. When this is the case, I know nothing better that we can do than, as Rush says, prescribe for symptoms, without being solicitous to give the disease a name. However, I think, if more attention were paid to these diseases, and they were faithfully detailed, it would be of singular service to the practitioners of medicine in general, and to the younger ones in particular.

Senac, in his invaluable Treatise of the Hidden Nature of Intermittent and Remittent Fevers, has given much very interesting information of those diseases under an irregular type.

When I first read his work, it was neither interesting nor pleasing to me; but in the autumn of 1807, after the influenza, I met with many cases of disguised intermittents, and, as soon as they appeared, I remembered what Senac said, and was much gratified, that a perusal of his work had enabled me to recognize immediately the disease. We never know how to justly appreciate any thing until we experience the want of it. So it is with respect to information. Such was my situation in regard to Senac's treatise; and had I not read it, should, with many of my fellow-practitioners, supposed this disease some other than it really was; and I do not believe that any practice would have been equally successful as that which I adopted, and which was suggested in consequence of the information derived from the perusal of Senac's works. Every case which I met with was soon remedied by the medicines proper in regular intermittents.

The causes of intermittents are generally known. In addition to the most common causes of that disease, there were, in 1807, concomitant ones. Whatever the cause of this disease may be, whether marsh miasma or any thing else, we know that, previous to its attack, there is a deficient excitement, and that the disease is of an asthenic order. Every person whom I saw labouring under this disease had previously the influenza; and all who had been thus affected complained of great general debility. In this city the influenza was less inflammatory than I suppose it to have been in many parts of America, consequently there was less occasion for much depletion. On another occasion, I have given it as my sentiment, that the too free use of the lancet frequently predisposed to the disguised intermittent.

Symptoms. The symptoms of the disease in question were very irregular, both in their appearance and in their time of appearing. There were some exceptions to this general rule. It most generally commenced with a slight chill, succeeded by excruciating pain, and terminated with a perspiration. No part of the body was exempt from pain, but the head was most frequently affected. In some it was confined to the forehead, in others it extended over the whole head. In some the occiput, or back of the head, extending down the neck, and in others one or the other of the sides of the head were only affected. In others the extremities, the sides, or the breast, were the suffering parts. In some cases, these pains only remitted; indeed, in a few cases, they were almost continual. At no stage of the paroxysms was there scarcely any increased arterial action, or other ordinary symptoms of fever; and that stage, which is generally termed the hot, should in this disease be termed the painful. From the disease frequently putting on a periodical type, although it was not very well marked, yet I was induced to suspect it a species of intermittent; and I was also induced to believe those of the same nature, whose prominent symptoms were analogous, even if they were less distinctively marked; and from there being little or no increased action in the pulse, from a perspiration succeeding when the pain either remitted or intermitted, and, finally, from the admirable success from the exhibition of bitters and tonics, I conceive my conclusion incontrovertible. Although the pain was in some cases so violent as to almost distract the sufferer, yet there seldom or never appeared in the parts affected any thing like congestion or local inflammation.

Treatment. Vesicatories were applied in a few cases, but their effect was very limited. Æther, laudanum, &c. as local

applications, had also a temporary effect. Those diffusible stimulants, which frequently break the chain of regular intermittents, were less efficacious in this disease; but bitters and tonics, when properly administered, never failed to cure.

We will now proceed to give a narration of the case which induced me to commence this essay.

February 15th, 1808, V. H. had for some time laboured under a severe pain in the left hip and lumbar region. This affection became so excruciating as to induce him to apply for medical aid. The physician who first saw him bled him and gave some powders, which I supposed to be a composition of antimony, &c. and directed a blister to be applied. But, in consequence of a severe pain immediately attacking the breast, with other acute symptoms of pneumonia, the blister was applied to this part, and he again bled; the powders continued. When I first saw him, the general symptoms were favourable: pain had nearly subsided, was nearly free from fever, skin moist, cough troublesome and dry. Tart. antim. et nitras potas. were given *ter in die:* this preparation kept up a determination to the skin, and also acted as a good expectorant.

16th. Was free from fever; pulse natural; bowels regular; there was a paucity of urine, with pain in passing it, occasioned, probably, by the vesicatory.

About the 20th, an affection of the stomach took place, resembling the pyrosis; there was also, at times, a discharge of blood from this viscus. As the aqueous discharge from the stomach was very considerable, to the amount of two or three quarts in twenty-four hours, I feared it would much debilitate my patient; consequently thought proper to arrest it: for

which purpose sal. tart. and a cold infusion of flor. cham. were given. Soon after commencing with these remedies, the discharge decreased; as soon as it had nearly ceased, a sense of tightness through the chest, with a disagreeable sense of distension and weight through the alimentary canal, supervened. From the time I first saw him, he had complained of similar sensations, when his bowels were not evacuated at least twice in twenty-four hours; when he had two or more dejections in a day, he felt light (to use his own expression); and when the affection of his stomach was suffered to return, it had a similar effect. It was not, therefore, desirable to stop it entirely, but to regulate it as the more important symptoms might indicate. For several days he was apparently much better; but as these symptoms declined, the pain in the hip and small of the back increased. He was now very solicitous to have a blister applied; from some disagreeable symptoms which occurred immediately after the application of the others, and which I supposed in consequence of it, I prevailed on him to defer this for a day or two. But the pain becoming exceedingly tormenting, and every other remedy proving ineffectual, I consented to the application of the blister.

26th. Called to see him before the blister was applied. The pain was excruciating; he had just been puking a large quantity of blood. There were other alarming symptoms. There was more appearance of inflammation than there had been for some time, but not enough to warrant bleeding, or other active depleting remedies, in his very reduced state. From the then prevailing symptoms, I feared a nephritis. The emp. episp. was applied, and he directed to take the antimonial powders.

27th. Irritability of stomach had subsided, and the discharge from it nearly ceased; the powders had excited a diaphoresis; blister drew well; had no pain in the back or hip. There was a troublesome ischuria; for which the following ischuretica was prescribed, and directed to be given as occasion might require:

℞	Tinct. opii.	ʒi.
	Gum. camph.	℈i.
	Vin. antim.	ʒi.
	Aq. font.	ʒij. M. f.

This preparation had the wished-for effect.

Occurred about this period a swelling in the left thigh and leg, accompanied with inflammation and great pain. The calf of the leg was very hard, and had much the appearance of an incipient suppuration. A solution of sacch. saturn. was applied.

29th. These symptoms had considerably subsided, and, in a few days, every appearance of suppuration ceased. The swelling continued. Stimulating washes were used to excite the absorbents to perform their proper functions: they had the desired effect.

Some months previous to this attack, V. H. received a hurt in the lumbar region; and he is naturally of a very delicate health.

G. Williamson.

Meteorological Table; and Statement of Deaths, with the Diseases and Ages, in the Borough of Harrisburgh, from the 1st of January until the 1st of December, 1808. *By Dr.* JOSEPH KELSO. *Communicated to the Editor by Dr.* JAMES.

It is to be regretted, that a prevalent error seems to be established in the public opinion, respecting the health of this place. Whether propagated through mistake or design it matters not; but so the fact is. It is to be hoped, the following statement will convince the public of their error, and place us in a situation as congenial to health, as any other in the United States. I hope, at least, it may have the desired effect, which is the only inducement I have for laying it before the public.

August, 1808.	Thermom. Morning	Noon	Evening	Barometer. Degrees A. M.	State of the Weather, A. M.
1	58	61	61	29.4	Clear
2	59	64	63	29.3	Foggy
3	59	65	63	29.4	Clear
4	62	65	63	29.4	Sunshine
5	62	63	61	29.3	Sultry
6	50	61	60	29.4	Cool
7	59	58	58	29.4	Rain
8	59	59½	59	29.2	Cloudy
9	59	58	59	29.3	Do.
10	58	52	60	29.3	Foggy
11	59	64	62	29.3	Do.
12	60	64	64	29.3	Cool
13	61	68	62	29.2	Cloudy
14	61	64	63	29.½	Do.
15	58	60	59	29.5	Clear
16	57	58	58	29.5	Sunshine
17	57	60	59	29.5	Foggy
18	58	61	60	29.4	Clear
19	58	62	60	29.3	Foggy
20	60	60	60	29.3	Clear
21	60	60	60	29.3	Foggy
22	58	62	61	29.5	Clear
23	58	64	63	29.4	Foggy
24	58	63½	63½	29.4	Do
25	62	63	62	29.3	Sultry
26	62	63	62	29.3	Windy
27	62	63	62	29.2	Do.
28	63	62	63	29.3	Heat
29	57	61	60	29.4	Foggy
30	58	60	60	29.5	Cold
31	57	59	60	29.6	Cool

September, 1808.	Thermom. Morning	Noon	Evening	Barometer. Degrees, A. M	State of the Weather, A. M.
1	55	60	60	29.5½	Cool
2	57	61	60	29.5½	Sultry
3	59	64	62	29.5	Do.
4	61	62	61	29.5	Rain
5	60	60	62	29.3	Sunshine
6	60	62	62	29.1	Do.
7	58	60	62	29.2½	Do.
8	57	60	60	29.6	Cool
9	57	58	58	29.5½	Foggy
10	55	61	60	29.5½	Do.
11	58	60	60	29.4	Do.
12	58	60	60	29.3	Windy
13	58	60	60	29.4	Sultry
14	57	63	60	29.3½	Cool
15	58	64	64	29.4	Do.
16	59	59	59	29.5	Rain
17	60	61	60	29.5	Windy
18	60	62	61	29.2	Do.
19	60	62	61	29.1	Clear
20	59	60	58	29.0½	Rain
21	55	68	55	29.3½	Windy
22	53	54	55	29.6	Cold
23	54	55	54	29.5	Windy
24	55	57	57	29.5	Rain
25	55	57	55	29.0½	Do.
26	53	55	55	29.2	Sunshine
27	51	53	55	29.4	Windy
28	50	53	55	29.5	Foggy
29	54	55	55	29.4	Rain
30	55	57	55	29.5	Sultry

October, 1808.	Thermom. Morning	Thermom. Noon	Thermom. Evening	Barometer. Degrees, A. M.	Barometer. State of the Weather, A. M.
1	57	59	57	29.4	Sultry
2	58	57	58	29.4	Rain
3	54	55	56	29.5	Clear
4	54	55	55	29.5	Cool
5	54	55	55	29.3	Foggy
6	54	55	54	29.2½	Windy
7	52	53	54	29.3	Cool
8	51	54	54	29.6	Foggy
9	55	56	56	29.5	Windy
10	55	57	57	29.2	Sultry
11	55	57	56	29.3	Foggy
12	56	57	57	29.4	Rain
13	55	57	58	29.4	Windy
14	54	56	57	29.5	Cool
15	56	57	56	29.4	Warm
16	54	55	54	29.3	Sultry
17	54	55	55	29.2	Rain
18	54	52	53	29.5	Do.
19	52	53	53	29.2	Frost
20	52	53	51	29.6	Rain
21	56	55	56	29.6	Sultry
22	56	57	56	29.4	Rain
23	55	56	57	29.3	Windy
24	54	55	54	29.6	Cold
25	52	53	54	29.6	Cool
26	54	55	54	29.4	Foggy
27	55	55	55	29.5	Cool
28	55	56	56	29.4	Windy
29	52	54	55	29.7	Frosty
30	53	54	55	29.7	Clear
31	51	58	59	29.8	Frosty

November, 1808.	Thermom. Morning	Thermom. Noon	Thermom. Evening	Barometer. Degrees, A. M.	Barometer. State of the Weather, A. M.
1	52	53	54	29.8	Foggy
2	53	54	55	29.7	Frosty
3	54	55	54	29.6	Sultry
4	55	56	57	29.5	Foggy
5	56	57	57	29.5	Do.
6	57	56	55	29.3	Windy
7	53	54	55	29.5	Cold
8	54	55	55	29.4	Cool
9	53	54	55	29.2	Sultry
10	54	55	56	29.2	Foggy
11	55	56	56	29.2	Warm
12	54	55	57	28.8	Do.
13	58	58	58	28.7	Rain
14	49	50	54	28.9	Cold
15	50	57	58	29.1	Snow
16	58	57	58	29.9	Windy
17	49	50	50	29.2	Do.
18	50	53	55	29.3	Cold
19	53	56	55	29.5	Frost
20	55	56	57	29.5	Sultry
21	53	54	55	29.5	Do.
22	56	56	55	29.2½	Do.
23	54	55	54	29.3	Warm
24	53	54	55	29.4	Windy
25	52	54	54	29.4	Do.
26	49	50	52	29.4	Do.
27	50	55	55	29.4	Calm
28	55	55	55	29.4	Hail
29	51	56	55	29.5	Cool
30	55	56	55	29.3	Do.

December, 1808.	Thermom. Morning	Thermom. Noon	Thermom. Evening	Barometer. Degrees, A. M.	Barometer. State of the Weather, A. M.
1	56	56	56	29.4	Sultry
2	57	56	56	29.5	Rain
3	55	56	55	29.1	Do.
4	55	55	54	29.3½	Do.
5	50	50	52	29.5	Windy
6	55	55	55	29.5	Do.
7	54	55	55	29.6	Snow
8	53	54	56	29.9	Do.
9	47	56	56	29.7	Frosty

DISEASES.	Under 2 years.	From 2 to 5.	From 5 to 10.	From 10 to 20.	From 20 to 30.	From 30 to 40.	From 40 to 50.	From 50 to 60.	From 60 to 70.	From 70 to 80.	From 80 to 90.	From 90 to 100	From 100 to 110	Ages unknown.	Total.
Amenorrhœa	0	0	0	1	0	0	0	0	0	0	0	0	0	0	1
Abortion	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Bilious remittent fever	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Convulsions	3	0	0	0	0	0	0	0	0	0	0	0	0	0	3
Consumption	0	0	0	0	2	0	0	1	0	0	0	0	0	0	3
Cholera	2	3	0	0	0	0	0	0	0	0	0	0	0	0	5
Cynanche tonsillaris	0	1	0	0	0	0	0	0	0	0	0	0	0	0	1
Dropsy	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Drunkenness	0	0	0	0	1	1	0	0	0	0	0	0	0	0	2
Dyspepsia	0	0	0	0	0	0	1	0	0	0	0	0	0	0	1
Hives or croup	3	0	0	0	0	0	0	0	0	0	0	0	0	0	3
Hysteria	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Infant. remittent fever	0	1	0	0	0	0	0	0	0	0	0	0	0	0	1
Locked jaw	1	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Pleurisy	0	0	0	0	0	0	0	1	0	0	0	0	0	0	1
Schirrus in utero	0	0	0	0	0	0	0	1	0	0	0	0	0	0	1
Teething	1	0	0	0	0	0	0	0	0	0	0	0	0	0	1
	12	5	0	1	7	1	1	3	0	0	0	0	0	0	30

Deaths in each month of the foregoing period.

	Adults.	Children.	Total.
January	2	2	4
February	1	0	1
March	1	3	4
April	0	2	2
May	2	2	4
June	1	1	2
July	1	3	4
August	1	0	1
September	2	1	3
October	2	1	3
November	0	1	1
December	1	0	1
	14	16	30

The population of the borough of Harrisburgh, is estimated at 2,500 souls and upwards.

The foregoing statement is drawn up with as much accuracy as possible, from the returns given to me from the sextons of both the English and German burying-grounds, and likewise from my own practice.

Harrisburgh, Dec. 12, 1808.

Omission. In the title of the Observations on Absorption, p. 55, insert By John Redman Coxe, M. D.